this fissure, on it cooling down, will be kneaded by mastication articles of irritating food and drink, which will lead to caries round it. The progress of this may *erode* a segment of the tooth, or at last penetrate into the cavity and the pulp, and lead to inflammation and abscess; and none is a more virulent agent in doing this than hot tea. The simmering kettle may be seen on the hobs of the kitchen fires in the houses of the working classes in Yorkshire and Lancashire, who are much subject to caries of the teeth and dyspepsia, in consequence of the frequent imbibition of its hot contents all day. W. G. BLACK.

Edinburgh, May 17.

The Lowell Observatory, Arizona.

I LEARN—although I have not myself yet seen the note—that NATURE has been unwittingly led into the error of stating that Lowell Observatory, at Flagstaff, Arizona, is a branch of Harvard College Observatory. This impression had its origin in a press dispatch, and I am, with the approval of Mr. Lowell, correcting these wrong impressions when possible.

Mr. Percival Lowell—whose father is trustee of the Lowell Fund from which the cost of the Lowell Institute Lectures is defrayed—is himself an author ("Japanese Art and Customs")and a man of scientific training. He has himself worked out the plan for his observatory work, and will personally supervise and direct the investigations. His institution is one of magnitude, having 18 in. and 12 in. telescopes, and he is justly entitled to the credit. The misunderstanding seems to have arisen from the fact that he has employed two of the Harvard College Observatory assistants for the season, they having been granted leave of absence. I enclose Mr. Lowell's own statement, published yesterday, being his paper before the Boston Scientific Society, and really the first public official statement.

May 26. JOHN RITCHIE, JUN.

[The following description is from the enclosure referred to by Mr. Ritchie. We are glad to render Mr. Lowell the credit due to him.—ED.]

The Lowell Observatory, the construction of which is now almost completed, is situated in the territory of Arizona, near the town of Flagstaff, in longitude 112° west, latitude 35° north, at an elevation of 7300 feet above the sea. Its site is thus the highest of any large observatory in the northern hemisphere, the next in point of elevation being the observatory at Denver, 5400 feet. In latitude it is furthermore the most southerly of those north of the equator. But the chief advantage hoped for from its position is in the way of atmospheric conditions, the singularly dry and clear climate of Arizona commending itself to astronomical purposes.

The observatory buildings stand upon the eastern end of a spur of high land, which rises just to the west of the town and is connected at the back some fifteen miles away with the San Francisco Mountains that reach to a height of 12,500 feet. The buildings are thus protected from the north. To the east and south they overlook the town and the plain beyond, being about 300 feet above Flagstaff and a mile away from it in an air line. The hill and the surrounding country are covered in part by a sparse growth of timber. Trees about an observatory are usually considered an advantage, as such vegetation reduces the radiation from the ground and tends to equalise the daily extremes of temperature, thus giving steadier seeing. The land for the site has very generously been given by the town, and a road to the observatory is being built by the town at its own expense.

The buildings consist of the equatorial building and of the study, placed at a short distance away from it to leeward of the prevailing winds. This disposition of the buildings is in order to minimise the risk from fire, a serious matter in so isolated a situation.

The dome of the equatorial building is constructed on a system of parallel arches, after a design by Prof. W. H. Pickering, who has made a study of domes here and abroad. It is built of a framework upon which rests a cage of wire-netting, and over this is stretched a covering of canvas. One of the chief features of the dome is its lightness. Although it is thirty-four feet in diameter, the whole revolving hemisphere weighs but two tons. Some idea of its lightness and of the ease of moving it in consequence may be got by comparing it with the dome of the large equatorial at Harvard, which, though four feet smaller, weighs fourteen tons, or seven times as much. The whole revolves on the wheels of a live-ring. The dome was built here and, together with the pier, shipped out in pieces to Arizona. The study building will contain a general or reception room, two sleeping rooms, a photographic room and a tool-room.

The telescopic equipment consists of three telescopes of 18 inches, 12 inches and 6 inches aperture, respectively. The 18-inch glass is by Brashear, and is the largest objective Mr. Brashear has yet finished. Its focal length is 26 feet 4 inches. This is an unusually long focus, and length of focus is an advantage in an objective. It and the 12-inch one of Clark's are mounted in twin. The 18-inch will be used for usual and spectroscopic purposes, while the 12-inch will be chiefly employed photographically. The third glass, the 6-inch, is also by Clark, and is a fine objective. It has already done good work at Flagstaff by being the first of those in the northern hemisphere to catch the Gale comet the other day. Incidentally, it is a far travelled telescope, having been safely half round the world and back again before ever it started for Arizona. It is of the same size and quality as the one with which Burnham made himself the first of double-star observers. By the ingenuity of Mr. Clark it is mounted portably in equatorial, being thus rendered the largest of small telescopes, or the smallest of large ones, at pleasure.

The 18-inch has been carefully fitted by Mr. Brashear with various ingenious contrivances by Prof. Pickering for photometric and spectroscopic work. For micrometrical purposes, in addition to the micrometer proper, he has also had prepared plates minutely ruled, dotted and designed and then diminished by photography, to be introduced beside the image in the telescope, for direct comparison with the canals and lakes of Mars and other similar purposes, thus furnishing a second method for micrometrical measurement of such detail.

The Berthollet-Proust Controversy and the Law of Definite Proportions.

IN his able address at the annual meeting of the Chemical Society, the President spoke of chemical text-books somewhat scornfully. While I confess that I am not prepared to regard these books as "soul-destroying," one and all, I have long felt at least that the dogmatic exposition of the elementary laws of chemistry to which they have accustomed us is most unsatisfactory, and that a critical re-statement of first principles is much needed. To deal with the subject fully, would carry me far beyond the limits of a letter to NATURE; but it is proposed in the following communication to draw attention to certain serious misconceptions which have crept into modern text-books with regard to the Berthollet-Proust controversy and the Law of Definite Proportions, and to attempt to re-define somewhat more accurately the points which were at issue.

Berthollet, it is said in the text-books, held that the composition of a compound was not rigidly constant, while Proust showed that "the same chemical compound always consists of the same elements combined in the same proportions by weight," (a statement to be referred to later, as statement A); and this statement is regarded as an enunciation of the Law of Definite Proportions, against the acceptance of which Berthollet strove so hard. As a matter of fact it seems unlikely that Berthollet would have felt in the least inclined to contradict the statement quoted. He did not suppose for a moment that it *was* possible for two substances to exist which should be sufficiently alike in properties for them to be called the same chemical compound, and yet for these to differ sensibly in their quantitative chemical composition; yet this is what a denial of statement A amounts to. On the contrary, Berthollet, like Proust, held the opposite view, namely, that the physical properties of substances are necessarily correlated with their chemical composition, and therefore that two substances differing in their chemical composition have in general different properties and are not called by the same name. We find Berthollet making use of this view, for instance, in the course of an argument given in the "Essai de Statique Chimique," vol. i. p. 346. For he says in effect that if in certain cases we only find compounds of which the constituents are united in ratios, such as $x : y$ or $x : y_1$, among the infinite number of compounds of these constituents capable of existing, it is just because these combination-ratios correspond precisely to some physical property (*e.g.* insolubility) which renders the resulting substance easy of isolation, and takes it (to use the terminology of the time) beyond the reach of the chemical forces which caused its formation.

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We see then that statement A in no wise discriminates between the views of Proust and Berthollet, and cannot be regarded as an enunciation of the Law of Definite Proportions. That such misrepresentation should have arisen, shows how hard it is to find in Proust's writings any sufficiently clear account of his views. I think the following may perhaps be accepted as a correct statement of his position:—

"If two substances unite chemically, they will do so either in a single ratio or in a series of ratios which are separated by intervals of finite magnitude." Berthollet thought, on the contrary, that in most cases two substances could combine chemically in an infinite number of ratios varying continuously between certain limits.

As Berthollet himself pointed out, the whole question turns on our definition of the term *chemical union*, and of *compound*, which latter we may regard as a substance formed by the chemical union of its constituents.

The joint efforts—or rather the divided efforts—of Berthollet and Proust made clear what neither chemist was willing to recognise frankly, namely, that there exist two distinct classes of homogeneous chemical substances which might fairly have been called compounds:—

Class I.—The substances belonging to this class are formed by the union of their constituents either in a single ratio or in a series of ratios separated by a definite interval. By the addition of a small quantity of any one of its constituents to a portion of a substance of this class we shall in general obtain a heterogeneous body.

Class II.—The substances belonging to this class are formed by the union of their constituents in an infinite number of ratios which vary continuously between certain limits. By the addition of a small quantity of any one of its constituents to a portion of a substance belonging to this class, we shall in general be able to obtain a new homogeneous substance.

Berthollet, on the one hand, regarded the substances belonging to Class I. as exceptions to the normal rule; Proust, on the other, wished to restrict the name of compound entirely to these. But for such a restriction Proust had certainly no sufficient reason to give. His point, however, as is well known, was carried by the weight of Dalton's discoveries of the Law of Equivalents and the Law of Multiple Proportions, which only applied to Class I. and therefore drew special attention to it, and by the weight of Dalton's atomic hypothesis, which allowed a sharp theoretical distinction to be drawn between the constitution of substances belonging to Class I. and that of substances belonging to Class II.[1]

To enter into a more complete discussion of this distinction as understood by Dalton, and of the modifications of the theory necessitated by our modern ideas on dissociation, would be beyond the scope of the present letter; but it should be pointed out that the controversy at present raging on the theory of solutions is, after a long interval, the continuation and development of the controversy between Berthollet and Proust.

With the facts clearly set before us, we may now inquire into the origin of the error of the text-books, an error which seems to me to be not a merely verbal one, but one due to a misunderstanding of the real points at issue.

I have not found this error in any book before Davy's "Elements of Chemical Philosophy," first published in 1812,[2] and it appeared but in comparatively few text-books until attention had been re-directed to the Law of Definite Proportions by Marignac and Stas in the "sixties."[3] It has since that time been reproduced in most of the books under one form or another. It is often stated, for instance, that Stas showed that "however we prepare ammonium chloride, it always has the same composition." The real problem attacked by that great chemist was this:—"Is what we have called 'chloride of ammonium' a single chloride of ammonium or a series of chlorides of ammonium resembling one another closely, and of which the combination-ratios only vary between certain narrow limits?" Or, to state the matter more generally, "Ought the substances which we have supposed to be members of Class I. (*supra*) to be really considered as members of Class II., with the reservation that the limits of the combination-ratios are in these cases close together?"

Stas's own language is, it must be admitted, not perfectly unambiguous on this point.

If Davy's treatise be possibly the historical source of the error, to trace its intellectual origin, or at any rate to offer some plausible explanation of this origin, we must go back further, and consider a time when a nomenclature founded on the exact chemical composition of substances, and the accurate measurement of their properties, was not so much impossible as uncontemplated.

In those days a common name was given to specimens of homogeneous substances, which might (excluding the case of the elements) prove to be either

(1) As in the case of liver of sulphur, a *series* of substances belonging to Class II.; or

(2) As in the case of water, a *single* substance belonging to Class I.; or

(3) As in the case of, say, some *particular* gun-metal, a *single* substance belonging to Class II.

I have chosen the order of these alternatives in this somewhat arbitrary manner in order to bring out more clearly the point where I believe misconception to have arisen. For I think that what the writers have done is to have considered only alternatives (1) and (2), and to have neglected (3).

After the rise of modern chemistry a closer examination of specimens of "liver of sulphur" and of "water" enabled chemists to say:

(B) The specimens labelled "liver of sulphur" do not always contain the same elements united in the same proportion. (C) The specimens labelled "water" do always contain the same elements united in the same proportion.

And these statements were contracted very naturally into the following:—

(D) "Liver of sulphur" does not always contain the same elements united in the same proportion; (E) "water" does always contain the same elements united in the same proportion.

Now the contracted statement (D) has misled the writers into forgetting that "liver of sulphur" is a collective noun, and that the contrast of the two statements is not a contrast between *a* mixture[1] and *a* compound, to which it is of course inapplicable; but a contrast between a *series* of mixtures and a *single* compound.[2]

They then proceed to substitute for "liver of sulphur" the general term, *a mixture*, and for water the general term, *a compound*, and so we obtain statement (F): *A mixture* does not always contain the same elements united in the same proportion; which is absurd; and our old friend (A): A *compound* does always contain the same elements united in the same proportion; which bears a very different meaning to that which its authors intend to convey by it, and which will be further considered immediately.

With statements F and A before them it appeared quite natural to the writers to suggest that "ammonium chloride" prepared in different ways might vary in composition, without their realising that they had returned to the careless days when "liver of sulphur" appeared an eminently satisfactory term.[3]

Perhaps from their error it may be possible to extract some good after all. For would it not be well to state more clearly in our books the postulate to which enunciation A reduces itself when interpreted rationally, viz. "Two portions of matter in other respects alike possess the same quantitative composition."[4] This postulate has been tacitly accepted by chemists, and it is made use of every day in the laboratory.

In conclusion I may be allowed to reply to a criticism which I foresee, namely, that no serious misunderstanding has followed from the errors to which I have drawn attention. That may be so, but accuracy of expression has a value of its own, and my object will have been attained, at any rate partially, if I have succeeded in removing (what should have been) a serious stumbling-block from the path of the student.

PHILIP J. HARTOG.

Owens College, Manchester, June 4.

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[1] Kopp in his contribution to the much transformed Lehrbuch of "Graham Otto," and Mendeléef in his treatise, depart very wisely from usual custom in calling both these classes of homogeneous bodies compounds.

[2] See "Davy's Collected Works," vol. iv., p. 279 (1840). This reference I owe to my friend Dr. Harden.

[3] It must be noted that the Law of Multiple Proportions includes the Law of Definite Proportions, and the latter has on that account often been omitted entirely from text-books.

[1] I use the word "mixture" for "homogeneous mixture," or "solution" in the sense of Van't Hoff.

[2] They have neglected alternative (3), which would have reminded them that E applies quite as well to any *single* homogeneous mixture as to any single compound.

[3] The accurate statement of the problem dealt with by Stas has been given previously.

[4] This interpretation may seem to some of my readers to need a fuller explanation, for which I have not space here.

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